begun at once and continued four-hourly. The urine cleared in thirty-six hours and the temperature dropped.

Besides the cases noted in this paper we have treated with vitex in Ranchi some 60 cases of malarial fever, and we have under treatment a case of blackwater fever which occurred at Rengari in a European nun.

This patient had recovered from the original attack of blackwater fever under treatment with *Aphloeia theaeformis*, and was brought into Ranchi with her urine still very dark and with abundance of bile in it, and obviously on the edge of a relapse. She was a very severe case to start with, and was in a very critical condition on arrival here. Under infusion of vitex (2 in 40) her urine absolutely cleared in thirty-six hours, her fever came down, and she has made a very good recovery so far.

Now to sum up the results generally. It must, in the first place, be strictly borne in mind that we are but at the very beginning of a very important investigation. It must not be forgotten that in the treatment of malarial fever with quinine—taking the average results of all practice—we get very varying results. There are cases treated with quinine which yield at once in the most gratifying fashion very shortly, if not immediately, after the commencement of treatment. There are others which yield after a week or so, others which persist longer, and others, again, of obstinate types which yield only after the most drastic treatment, or after prolonged treatment, and we are tempted in some few of these latter to ask what the real effect of quinine has been, and whether it has really had any effect. We are accustomed to regard quinine as the specific for malaria, and in prolonged cases yielding only after an obstinate fight we say in practice that quinine has succeeded only after a determined exhibition of it. I am referring now to cases of undoubted malaria. Now, with vitex my experience has been that we have had the same series of varying results—namely, cases that yield at once, others which yield after a short period, others which persist, and others which yield only after a prolonged struggle; and we have had with vitex no experience at all comparable to that of the giving of alkaloids of quinine hypodermically or by intravenous injection. All this, as regards quinine cases, is familiar to most men practising in malarial countries.

There is, however, one very important point of difference at this stage of our investigation, for with quinine we have the finished article worked out to a nicety. The original article was a wild South American forest plant, and now we have all its alkaloidal products differentiated, and the alkaloids have doses that are standards unto themselves and recognized as such. We know the clinical standard value of each of them, and when we come to use them we more or less know what we are dealing with—more with reference to the chemical constitution of the drug we are using, less with reference to what I may perhaps allude to as the coefficient of virulence and resistance to drugs of the particular malaria under treatment. With these two opposing factors we get our varying clinical results with quinine and its allied alkaloids—that is to say, with a series of drugs which have something of a recognized clinical standard value of efficiency. On the other hand, vitex is at present in the stage approximately in which cinchona was when first discovered in the Peruvian forests. The cases I record are the very first fruits, and the singular value of these first observations lies in (1) the extraordinary parallel in the types of the results of treatment with quinine on the one hand and vitex on the other; (2) the extremely encouraging general result of cure which, so far, has practically been as uniform as with quinine. It must be borne in mind that vitex is now in the position in which cinchona was when first discovered—a drug used only in the crudest way by aborigines. The way in which I have so far used it is the aboriginal method. Its results have been good.

I would next point out certain further details and differences.

1. Quinine is extremely bitter; vitex is not so.
2. Quinine produces many troublesome effects and after-effects which I need not dilate on, and is toxic. Vitex produces nothing approaching quininism, and seems to be absolutely non-toxic, and, so far, has given no unpleasant after-effects.
3. Quinine is known to have definite depressing effects on the heart and circulation; vitex, after its use, produces a feeling of well-being, is mildly stimulant, appears to have no depressing effect on the heart, and has a clear diuretic effect.
4. Quinine is oxytocic, and there are dangers in giving it in pregnancy. Vitex, as far as our experience of it goes, has apparently no such dangers attending its administration.

It would appear, therefore, that in *Vitex peduncularis* it may yet be found that we have a most valuable remedy for malaria, and that all that is needed is to work out its properties in detail.

I would add one interesting point which, however, needs investigation. Mr. McLeod Smith, the subdivisional officer of Simdega, tells me that during the last outbreak of influenza in 1918–19 those villages that were accustomed to use this plant for malaria used it in the influenza outbreak and had a mortality definitely lower than those other villages which did not use it. This is but a loose observation from a layman, but acting on it I have so far used it in a few cases of obvious influenza which have recently occurred, with a complete relief resulting after three doses in twenty-four hours of the 2 in 40 infusion of leaf. I give this for what it may be worth as an isolated observation, but I shall push it if opportunity offers.

The root bark and stem bark are also about as useful as the leaves for all purposes for which the latter may be used. This is to a large extent confirmed in my experience.

The infusion of leaves and bark is said to be useful in cystitis. I have found it so in one case. I certainly find it an excellent diuretic.

In vitex it may be that we shall find active principles which will give us all the power of quinine in malaria without many of the dangers and drawbacks of quinine and its allied alkaloids, and we have the most important additional clinical value of this plant in blackwater fever, the scourge of our forest areas and of the submontane tea districts. If future investigation confirms the value of this plant in malaria, it must be strictly preserved in the forest areas of the country. It is widely distributed, perhaps, but none too abundant where it occurs. One may find a hundred plants on a single acre, and travel over miles of forest before he comes across another plant.

Finally, I must express my obligations to all who have helped me in this investigation so far; to Kuril Kerketta of Kuruskela in Ranchi District, my aboriginal informant, who put me on to the leaf and plant; to Mr. David McLeod Smith, subdivisional officer of Simdega in this district, for his keen all-round help, and for much information as to the aboriginal and local names of the plant and its local uses, and for collecting the leaves and bark; also to Mr. E. Durham-Waite for the first clear botanical specimens put up; and to Miss McDonald and Dr. Arit of the Society for the Propagation of the Gospel Mission Hospital, and to Mother Henriette of the Ursuline Convent at Ranchi, and Assistant Surgeon Garib Das Gupta at Ranchi, and Subassistant Surgeon Khudiram Mukerjee of Simdega, for all the help they have given me in various ways in clinical work, and in collecting leaf and bark.

DETAIL OF PREPARATION SO FAR USED.

1. *Infusion of leaf*—in three strengths, as detailed in the foregoing notes—1 in 40, 2 in 40, 4 in 40.

2. *Concentrated infusion of leaf.*

Powdered leaf	8 oz.
Sp. vin. rect.	2½ oz.
Aqua chloroformi	ad 12 oz.

Make up to 12 oz. with aqua chloroformi after a week's percolation and straining off liquid. *Dose:* Half to one ounce every four hours. Found as good as leaf infusions (1).

3. *Fresh stem bark extract.*

Ground bark	4½ oz.
Sp. vin. rect.	6 oz.
Aqua chloroformi	ad 30 oz.

Percolate for a week, and make up liquid with aqua chloroformi to 30 oz. *Dose:* Four to six teaspoonfuls every four hours. Found about as effective as leaf infusions (1).

DILATATION OF THE HEART.

BY

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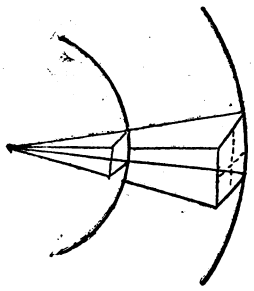
THE mechanical disadvantages under which a dilated heart labours are not likely to be adequately realized till the influence of size on the working conditions of the heart is more fully grasped. The heart is commonly thought of as so much muscle, better or worse nourished, and better or worse controlled; whereas the factor of the size of the chamber it encloses is largely overlooked, though of immense importance to its working.

Now, if four lines be drawn from the centre of two concentric spherical surfaces through both surfaces, as in the diagram, the diameter of one sphere being double that of the other, the area enclosed between the points of passage of the first surface will obviously be but one-quarter of that for the second.

If a sphere dilate, therefore, to twice its diameter or radius, it would have an internal surface area four times as large as before, and its walls, to cover that greater surface, would necessarily become four times as thin. It may be objected that the heart is not a sphere; the same principle, however, applies, so that if the heart were dilated to double its diameter its inner surface would be approximately four times as large, and the heart walls four times as thin. For this reason alone it would be roughly four times as difficult for the heart to contract on its contained blood, there being four times the area of blood to contract on, or four times the number of units of area of blood to resist its contraction.



This, however, is not the whole difficulty, for the heart's power of exerting a pressure on its contents becomes halved when its diameter is doubled, so that while the work to be done is quadrupled, the working power is halved, or, in other words, the heart's embarrassments are increased eightfold.

The proof that the working power of the heart is thus lowered may be presented as follows: The force with which a hollow rubber or muscular sphere can contract on its contents is merely the force with which one-half of the sphere can pull the other half towards itself, and is measured by the pull across any circumference.

If p = the pull of a unit of muscle;
,, c = the length of circumference, which is $2\pi r$, where r is the radius;
,, t = the thickness of muscle at the circumference;
then pct = the pull across the whole circumference drawing the two halves of the hollow sphere together, and so compressing its contents.

Now pct is the same as $p2\pi rt$, but I have already pointed out that t, the thickness, becomes four times less when r, the radius, is doubled.

When, therefore, the sphere is doubled in radius or diameter the pull across the circumference is doubled in virtue of the circumference being double as long, but it is one-quarter in strength in view of the fact that the muscular material is only one-quarter as thick. On the whole, therefore, the pull is halved.

If the sphere were further dilated, say to three times its former radius, the pull would amount to $p\ 2\pi\ 3r\frac{t}{9}$, or, in other words, it would gain threefold by the increase in circumference, and lose ninefold by the decrease in thickness, since the thickness decreases inversely as the square of the radius, as already shown.

Though the heart is not a true sphere it is a hollow roundish viscus, and, in its measure, is subject to the same physical principles. When enlarged it has more units of surface of blood, increasing, more or less, as the square of the diameter to resist its pressure; and it has less power to contract, the power changing in inverse ratio to the diameter.

From these two causes combined, the compression the heart can exert on its contents would decrease eightfold if the diameter of its chamber were doubled, and twenty-seven-fold if the diameter were trebled — that is, it varies always inversely as the *cube* of the diameter or radius.

As applied to the heart or any hollow viscus in the body, such as the bladder or uterus, these figures must not be taken as of any definite value, other than showing a physical principle to which they are subject, and the immense mechanical drawback of dilatation.

This consideration shows why "breaking the waters" when delivery is well advanced is followed by such powerful contractions on the foetus. It is quite likely that the contractions are no more powerful than before, but merely more efficient. The surface to contract on is smaller, and the two halves of the uterus can be drawn towards one another with more force, simply because the uterus is thickened. With less to contract on and a thicker contracting wall no increased muscular effort is required to produce an enormously increased expulsive effect, the pressure on the foetus could, more or less, rise inversely as the cube of the decrease in diameter.

A dilated heart may be greatly relieved by blood-letting, because it allows the distended ventricle to contract; and, once contracted, the muscle wall may, at least for a while, prevent its chamber from becoming over-large again, for it is much easier to keep a chamber small than to reduce it when once large.

I have endeavoured to show in many papers in the BRITISH MEDICAL JOURNAL and elsewhere, in the past, that the same physical principle applied to the auricle probably explains most that is puzzling in the behaviour of the heart in mitral stenosis. The presystolic crescendo murmur and its running up to and into the first sound, the absence of ventricular regurgitation through an incompetent mitral valve, the presence of regurgitation and a systolic murmur replacing the presystolic when the auricle later on dilates and allows regurgitation into it, and so on.

The important thing, however, to first realize is that the size of the cardiac chamber at any time determines, more than is readily imagined possible, the power of the heart to empty itself, the heart weakening, as it dilates, inversely, more or less, as the *cube* of the dilatation.

A STUDY OF THE PNEUMOCOCCUS AND STREPTOCOCCUS GROUPS IN THEIR RELATION TO INFLUENZA.*

BY

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(Abstract of Report to the Medical Research Council, from the Research Laboratory of the Royal College of Physicians, Edinburgh; Professor James Ritchie, Superintendent.)

THE object of this investigation was a study of the pneumococci and streptococci from cases of epidemic influenza, firstly in their relation to the earliest stage of illness, when the infection was presumably a comparatively pure one; and, secondly, with regard to their part in the more advanced cases, when pulmonary complications had set in.

The preliminary portion of the inquiry was directed to questions of technique. It was necessary to define precisely what constitutes a pneumococcus and what a streptococcus, and what criteria may be adopted for distinguishing various types of these organisms from one another. It was concluded that, while there are various properties which, if taken together, may serve to distinguish the pneumococcus and streptococcus groups from each other, there is only one test which, if used as a single test, can serve to differentiate the two groups—the bile test. The solubility of pneumococcus in bile, apparently an empirical reaction, is nevertheless an indication of a specific chemical constitution of the pneumococcus, which it does not share with the streptococcus group. This test was then adopted as the invariable means of distinguishing the two groups, while the other tests in common use—inulin fermentation, presence of capsule, type of colony, morphology, virulence to mice, power of growth in certain media, homogeneity or granularity of growth in plain broth, etc., were used only as secondary confirmatory tests.

The methods adopted for differentiating the different types of organisms within these two groups were for the pneumococci serological, agglutination tests with the Rockefeller Types I, II, and III serums being carried out; for the streptococci, biochemical, fermentation and haemolytic tests being used.

The cases studied may be taken as typical of those occurring in the Edinburgh district during the second and third waves of the epidemic, that is, during those outbreaks which reached their height in November, 1918, and in February, 1919. One hundred and eight cases were studied completely from the point of view of their

*Advance reports on this subject have already been communicated as follows: (1) Preliminary Report to the Medical Research Committee, April, 1919; (2) Paper read before the Edinburgh Medico-Chirurgical Society, May 14th, 1919, and published by the *Edinburgh Medical Journal*, July, 1919; and (3) Paper read before the Pathological Society of Great Britain and Ireland, July, 1919; the Report (4) of which the present paper is an abstract was sent to the Medical Research Committee in April, 1920, and will be published in the *Edinburgh Medical Journal*.